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CROONIAN LECTURES,
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IN MDCCCXXXIII,
ON
CHOLERA;

BY
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PREFACE.

I SUBMIT these Lectures to the Public because I conceive it to be the duty of every medical man who may have had particular opportunities of witnessing a disorder, the nature or treatment of which may be doubtful, to communicate to the Profession any facts respecting it, which may appear interesting or important. It was my chief object during the prevalence of Cholera in this City, to avail myself of the facilities afforded me by the appointments which I held at that time, of noticing the various plans of treating the disease, adopted in different Institutions, in order to discover the most efficient means of giving relief, what remedies to resort to, when to employ them, and if possible to ascertain the manner by which benefit, if obtained, had been derived. Having by this means detected, as I believe, some sources of error, and acquired some useful information on the subject, I adopted it for the Lectures which I had been appointed to deliver in the Spring. These I have determined to publish now that Cholera has reappeared ; trusting that what I found serviceable in my practice, will prove equally so in the practice of others, and claiming only the merit of stating with impartiality and good faith the result of my observation and experience.

LECTURE I.

I CHOSE "General Pathology" as the subject of the Croonian Lectures which I delivered last year; I then discussed the principal Causes of Disease, and the alterations in structure to which the tissues of our bodies are exposed. The next subject of enquiry which I propose to myself to adopt, is the consideration of those particular diseases, which have at various periods prevailed extensively; for by regarding in a comprehensive manner these inflictions on the human species, it seems to me, that more just and proper notions of their origin will be derived, than when we confine our observations within narrower limits:—in which case *causes* are likely to be overlooked, and *effects* mistaken for or confounded with them. Not indeed that the history of diseases affecting large masses of mankind has been neglected by those, who flourished at the periods in which they raged; but the description of them has often been undertaken by persons who being themselves unskilled in medicine, and unable to judge of the theories which they advocated, have perpetuated erroneous notions and obscured the truth by their marvellous relations. It is moreover useful in

this advanced stage of our knowledge, to observe the sources of error of former ages, in order that these may be avoided, at the same time that we may check the natural proclivity to run into opposite extremes. The narratives of various historians singularly agree in referring to the same causes the origin of the numerous and dissimilar pestilences, which have extensively prevailed; and even at the present hour we find that notions founded upon ancient superstitions, are not without their supporters.

The pathologist finds in the class of epidemics the most fatal disorders, as well as those which have extended to the remotest regions of the earth. Our own country fortunately, although it has suffered severely from the frequent introduction of such visitations, seems not to have been the birth-place of any one which has extended itself in a formidable degree. The low typhus-fever of this climate, is of all contagions the most manageable, the most readily arrested, and most easily eradicated.

Every age has attributed to Æthiopia and to Egypt the original source and the seminary of plague. To these countries Thucydides dates the origin of the pestilence which raged at Athens. We read that the fatal plague which depopulated the earth in the time of Justinian and his successors, first appeared in the neighbourhood of Pelusium, between the Serbonean Swamp and the Eastern channel of the Nile; thence it spread to the East over Syria, Persia and the Indies: it penetrated to the West along the coast of Africa and over the

continent of Europe. In the spring of the second year it reached Constantinople, where its symptoms and progress were accurately observed by Procopius. By degrees the malignity of this plague abated and it dispersed; but it was not till the end of a calamitous period of 52 years that it could be said to have been completely subdued. From this situation it has repeatedly emerged, and seems there at all times to be domesticated.

Italy appears to have produced two at least of the most common of the epidemics, Scarlet Fever and Measles; and to Ingrassia of Naples, the credit of their first description is to be given. These fevers, although at the present day much within control, have yet at different periods, equalled in fatality, diseases which are now more destructive.

The Sudor Anglicus or Sweating Sickness raged at Dunkirk, was imported into our island by the ill appointed armament of Henry VII. and remained amongst us for nearly a century.

The Yellow Fever has as yet confined its ravages to the West Indies and Spain, without approaching nearer to our shores than the frontiers of that kingdom.

The disorder which appears to have spread most extensively over the globe, was the Catarrh, which originated in China in 1781. This diffused itself throughout Asia, and after visiting this country extended as far as the continent of America. Nearly similar has been the march of the epidemic Cholera,

to the consideration of the progress of which in England, I propose to devote this and the two succeeding Lectures.—

In selecting this subject I know that I have chosen one which has engaged the thoughts of many and on which the opinions of the most distinguished members of our profession continue much at variance. Under these circumstances I feel some hesitation in expressing any ideas upon it, and only venture to do so from having enjoyed numerous opportunities of witnessing the disease as one of the medical officers attached to the City of London Board of Health, as the physician entrusted with the charge of the Cholera patients at St. Bartholomew's Hospital during the months of June, July and August, and as the physician to the floating Hospital for Seamen, the Dreadnought.

In speaking of the disease lately introduced amongst us, I shall designate it by the term malignant, as more appropriate than spasmodic: first, Because of its fatal tendency, next, Because English Cholera is spasmodic, and thirdly, Because the more severe form of the disease is not attended by spasms.

So much has been written on its history, that I shall confine my remarks on this head to those statements in various authors which appear to me to be erroneous.

It has been asserted that Hippocrates was acquainted with it, but he mentions fever as one of the early symptoms of the Cholera with which he was familiar; it is evident, therefore, that he does

not describe malignant Cholera; as fever is not one of the early symptoms of that disorder.

Nor is the supposed knowledge of this disease attributed to Aretæus, more accurate, who, in relating the symptoms of the malady with which he was familiar, and describing the evacuations in that form of Cholera, says, “*ιδεην δε τα πρωτα μεν υδατωδεα τα δι’ εμετου, τα δε δι’ εδρης κοπρανα, υγρα, δυσωδεα,*” thus marking at once the difference between the disease then prevailing and the malignant Cholera, in which, as is well known, the dejections are free from admixture with fæcal matter.

It cannot be necessary to dwell on the difference between the Indian Cholera and that which is incident to our own country and others within the same degrees of latitude, which occurs during hot weather only, and which Sydenham calls “*morbus qui æstatem fugientem atque autumnum imminentem complectitur, unicè ac eâdem prorsus fide, qua veris primordia hirundines, aut insequentis tempestatis fervorem, cuculus amare consuevit.*” One remark, however, may be made, viz., that the month of August which he thus fixes as the ordinary period for the appearance of common cholera, is certainly not now the month, when that disease generally prevails; as it is just as frequent in September as in August, and more so in June and July than in either.

It is possible that some cases of this our indigenous complaint may be referable to indulgence in fruit, but many certainly arise without any such exciting cause, and it is probable that these

periodical affections may be more satisfactorily explained by the changes, which Dr. Edwards has shewn to take place in the animal economy at different seasons of the year, and which are indicated by the quantity of nitrogen given off during respiration, thus, perhaps, adapting the system to extreme variations of temperature. Of the actual origin of Asiatic cholera, which has been by most authorities dated to Jessore, in 1817, some doubt may be entertained, both as regards the time and place of its first appearance. Without quoting writers of questionable authority, it cannot, I conceive, be doubted that the description of the disorder given by Mr. Curtis in the Madras Reports as prevailing in the year 1782, must have reference to that form of Cholera lately disseminated over the world. In the account alluded to, the disease is described, 35 years before it became epidemic, as spreading in Sir Edward Hughes's squadron, then stationed in the East, and as having arisen from communication with an infected port in Ceylon. Eight cases occurred on board one of the ships in which Mr. Curtis was surgeon; and he says they so much resembled one another, that the description of one might suffice for the whole; any slight difference which might occur being only in the suddenness of the attack, or the rapidity in the succession of the symptoms.

His account is somewhat long, yet he so accurately describes the disease lately introduced here as to satisfy me of his acquaintance with it.

“ The disease,” he says, “ began with watery purging, attended with some tenesmus, but with little or no griping. This always came on some time in the night, or early in the morning, and continued some hours, before any spasms were felt ; and slight affections of this kind being very common in the country, the patients seldom mentioned them till they began to be more severe, and extended to the legs and thighs. The purging soon brought on great weakness, coldness of the extremities, and a remarkable paleness, sinking and lividity of the whole countenance. Some at this period had nausea and retching to vomit, but brought up nothing *bilious*. In a short time the spasms began to affect the muscles of the thighs, abdomen and thorax, and lastly, they passed to those of the arms and fingers. The rapidity with which these spasms succeeded their first attack, and their severity, especially as affecting the muscles of the thorax and abdomen, denoted in general the degree and danger in the case. The affection is not as in tetanus, confined to a single or to a certain class of muscles only, neither does it as in the spasmus clonicus move and agitate the members : it is a fixed cramp in the belly of the muscles which is gathered up in a hard knot with excruciating pain. In a minute or two this relaxes, is again renewed, or the affection passes to others, leaving the miserable sufferer hardly an interval of ease. As the disease proceeded,” the account continues, “ the countenance became more and more pale, wan and dejected, the eyes sunk,

hollow and surrounded with a livid circle; the pulse became more feeble and sometimes sank so much as not to be felt at the wrist, in two or three hours after the spasms came on; but so long as it could be felt it was but little altered in frequency. If the spasms happened to remit, it would sometimes rise a little and the countenance assume a better look. The tongue was generally white, and more or less furred towards its root: the patients had all great thirst, or rather a strong desire for cold drinks: but there was no headache or affection of the sensorium commune throughout.

“The coldness of the extremities which was perceptible from the very first, continued to increase and spread over the whole body, but with no moisture in the skin, till the severity of the pain and spasms forced out a clammy sweat, which soon became profuse. The hands now began to put on a striking and peculiar appearance, the nails of the fingers became livid and bent inwards; the skin of the palms became blanched and wrinkled up into folds as if long soaked in water; the effect, no doubt, of the profuse cold-sweat, which is one of the most pernicious and fatal symptoms of the disease, both from the effect which it has in such a climate of exhausting the strength and of abstracting heat from the system. In some of the present cases, and in many others, after this we had recoveries from the severest degree of spasmodic affection; even where the pulse had been for hours completely lost at the

wrist, and the body perfectly cold ; but never of any who had these profuse cold clammy sweats, and where the hands had put on this appearance.

“ All this while,” Mr. Curtis adds, “ the purging continued frequent, and exhibited nothing but a thin watery matter or mucus. In many, the stomach became at last so irritable, that nothing could be got to rest upon it, but every thing that was drank was spouted out immediately, without straining or retching. The countenance and extremities became livid, the pulsations of the heart more quick, frequent and feeble ; the breathing was laborious and panting : and in fine the whole powers of life fell under such great and speedy collapse as to be soon beyond the power of recovery.

“ In this progression the patient remained from three to six hours from the accession of the spasms, seldom longer ; these began at last to abate, but with more internal oppression, great jactitation and the motion of the heart so long as it could be felt became more and more feeble, quick and irregular, till death came to the relief of the patient. Sometimes before this event took place the spasms gradually abating left the patients entirely, and they retained so much of their faculties that they would continue to talk sensibly to their messmates to the last moment of their lives.”

It were unnecessary to insist on the actual identity of the disease just described, with that recently prevalent in England. It is clearly the same, and appeared to spread by communication with infected ships.

Mr. Barnes, who was the medical superintendant at Jessore from the year 1810, mentions, in a letter addressed to me as one of the medical officers of the City Board of Health, that he had become acquainted with the disease since he had joined the station. That it was to him entirely new, and one which had superseded the periodical remittent fever, formerly endemic. He states that twice previously to 1817 this disease prevailed to such an extent in Jessore and its immediate neighbourhood as to render it necessary to shut the courts of justice and suspend all business for a considerable length of time. The origin of this monstrous birth may then be dated earlier than the period usually assigned to it.

Its origin too as an epidemic may be ascribed to other places than Jessore, as Dr. Christie relates that it existed in the various parts of the southern districts of Bengal, in the months of May and June, 1817; that it did not create alarm in Jessore till August; that several cases took place in Calcutta on the 5th of September; and from that day forward the disease became daily more frequent.

It is thus probable that it may have had more than one source; but wherever generated, and however conveyed, it seems unquestionably to have been long known in the East, but its power of extension till now has been comparatively limited. At length, however, setting at nought all human precautions, it has rivalled in extent the widest spreading contagion. Obeying the laws

which had previously regulated its advance it appeared in Hamburgh, from whence the communication is frequent with our northern ports, in one of which, it first appeared on our Islands.

Although we consider Hamburgh as the infected port whence the disease was communicated to this country, it does not appear that the disease shewed itself in those who thus conveyed it. Such cases, however, are by no means singular in the history of this disorder. At Orenburg it could only be inferred that it was introduced by infection; its actual introduction could not be discovered; and similar remarks had been made in many other places. Commencing its route in the county of Durham, the Cholera proceeded by slow advances towards the north, travelling at a rate of but little more than 100 miles in three months. Its next appearance was in the southern ports, occurring amongst those persons immediately connected with, indeed actually in many instances belonging to, infected places in the north. From these extreme points, as was the case in Persia, the disorder gradually advanced from the confines to the centre of the kingdom. Its introduction into this country and its progress through it were strictly analogous to its former course.

I shall now make some observations on the characters which it has assumed.

Mr. Curtis, in his description of the disease as observed in India, mentions several varieties of it—one distinguished by the absence of vomiting and

the prevalence of diarrhœa ; another by the excess of vomiting and occasional absence of the dejections ; and a third by very slight commotion in the system indicated either by the evacuations or by spasm : this last, he says, he considered the least manageable of all. The form in which it most frequently presented itself to him, was that in which the alvine dejections were particularly frequent. There are, however, two other forms which this disorder has occasionally put on. One in which spasms alone have been observed, and another in which copious determinations of blood have taken place to the thorax and abdomen. Of the former of these I have met with but few examples in this country ; they were however more frequent in India, the practitioners having repeatedly noticed this variety ; and this in all probability is the convulsive disorder noticed during the prevalence of Cholera, and denominated by Girdlestone and others “ Idiopathic Spasm.”

But the other form, that, namely, characterized by large effusions of blood, is but rarely referred to by foreign writers, if, indeed, distinctly described by any, though a form which it seems to me clearly to have assumed, and is one which may perhaps throw some light on the actual nature of the complaint.

Here, as on the Continent, ingenuity has been exercised to find sources of this plague, and the means by which it has been communicated. Mr. Barnes, in the letter already alluded to, in speak-

ing of its earlier appearance in Jessore, describes the state of that town as one fully capable of generating pestilence ; shewing that sanitary regulations form but a secondary consideration in the minds of those who have the superintendence of the native towns in India. To the infectious exhalations from the pits which surround the habitations of the natives, and where all refuse is thrown and all filth deposited, has generally been ascribed the commencement of the disease. But although undoubtedly the products of vegetable and animal decomposition prove actively poisonous, yet other circumstances must have conspired to produce this peculiar effect : such a train of consequences at least has not been observed to result from vegetable exhalations simply—nor have such symptoms been produced by exposure to animal effluvia, in a state of decomposition, nor from the combined effects of both.

The state of the atmosphere, however, much influences the extension of cholera, and the years 1816 and 1817, previous to its earlier migration, were observed to be unusually wet. The same remark was made in this country before it appeared among us, confirming the established notions on the propagation of disease in wet seasons, especially when increased temperature has been conjoined with moisture. On the first approach of the Cholera to our island, the weather was mild for the season, with more than usual rain. A moist and warm October, was succeeded by a foggy month of November, and the same facts were

noticed in February 1832, when it first appeared in London.

The meteorological journal kept by the Horticultural Society, shows, that during this month fogs were very generally prevalent: and that the degree of moisture in the air then and during January, was unusually great, exceeding indeed, that commonly observed in November and December; during March the same remark was made, nor did the air recover its usual dryness until the end of that month.

June was remarkably warm, and the moisture in the atmosphere was above the mean. During this month (June) nearly three inches of rain fell, and the mean temperature was two degrees above the average.

The month of July was hotter than is customary. In August, double the usual quantity of rain fell; and the temperature was three degrees higher than common. September was hot, but fine. October was wet.

Thus increased temperature and unusual moisture, characterized the weather during the prevalence of Cholera. The heat during the summer months indeed, was so considerable, as to influence the mean temperature of the year, which was above the average.

The *mean* of barometrical observation presented nothing extraordinary; but at one time, on the 28th of August, the mercury was lower than it had been for six months previously, and then numerous cases occurred. The barometer then

reached 29.18, and the fluctuations must have been considerable, as the mean pressure of the atmosphere in the month was above the average. During the week in which this fall in the barometer took place, the bills of mortality gave an increase of 477 in the number of deaths, as compared with the preceding ones. There may however be other causes for this, the period of the year may itself account for it. August and September were the months in which the Plague of 1665 proved most fatal. In the whole of the month of July of that year, 4000 persons died; but Evelyn in his Memoirs, makes this memorandum on the 8th of August: "died this week 4,000;" on the 15th, "died this week 5,000;" and on the 7th of September he writes, "nearly 10,000 are perishing weekly."

The simple fluctuations of temperature and variation in the dryness or moisture of a season, are insufficient, however, to account for the origin of any specific contagion, although no doubt favouring its increase and dissemination. These variations we may presume to be entirely accidental. Indeed, during the prevalence of some of the severest pestilences, the seasons have proved remarkably fine. Tacitus, in speaking of the Plague which raged in the time of Nero, which visited every house and thronged the public ways with funerals, attacking all classes, and sparing neither age nor sex, says, there was no obvious disorder of the season. "*Nulla cœli intemperies.*" During the pestilence

which raged in Paris in 1580, and destroyed 40,000 of its inhabitants, the air was so serene and clear, that it was necessary to seek for other causes for the epidemic than the atmosphere. And Sydenham alluding to the Plague in 1665, says, that the season was temperate and calm, and the fruit came to its perfection. Other arguments might be brought forward against the origin of contagious maladies from any obvious disorder in the atmosphere; as every variety of season has recurred, without any such consequence; and should any essential alteration take place in the constitution of the air, we surely might expect to find that the vegetable kingdom, most immediately dependent on physical impressions, would suffer in a corresponding degree, but no such effect took place in plants during the existence of the late epidemic. True, it has been remarked abroad, that at that time vegetable matter decomposed with surprizing rapidity, and such a proclivity has been observed in England; but not from any disordered or unwholesome state of the air, for the fruit ripened well, and vegetables attained their full perfection. And in this circumstance indeed, we find the very cause of their decay, for having arrived at complete maturity, they tend naturally to more rapid decomposition.

It does not follow, on the other hand, because the state of weather is so ungenial as to check the growth, or occasion the withering of plants, that an epidemic must simultaneously

spring up ; one certainly has in general succeeded, but only as the consequence of a time of scarcity or famine, and not in consequence of the condition of the air. Much has been done in estimating the weight and elasticity of the atmosphere, much yet remains to be investigated concerning the casual additions to its ordinary components, oxygen, azote, carbonic acid and vapour.

Amongst other theories, that of Mr. Orton demands some notice ; but the calculation of the cases which fell under my own more immediate notice, was decidedly against the lunar influence which he supposes to operate in the propagation of this disease. He concludes that the phases of full and new moon were the morbidic periods, while the first and last quarters were comparatively harmless ; but in July during three days before and three days after the commencement of the first and last quarters in the non-morbidic periods, 196 cases occurred ; while in the same space of time previous to and after the new and full moon, the supposed morbidic period, only 55 cases were reported. Again in August, in the non-morbidic period, there were 56 cases ; in the morbidic one, 46. In September too, in the non-morbidic days there were 49 cases, in the morbidic ones, 26. Clearly showing that those days when the tides were high and swift, were not the most pregnant with this disorder ; and that should any inference be drawn from a few cases, it would be in direct opposition to the theory of Mr. Orton.

It were tedious to dwell on other theories : emanations from the earth, the ire of the gods,

the unfavourable conjunction of the planets with their casual obscurities, have been advanced from the earliest ages, and have been again in turn revived. And it is even to be feared, that to propitiate the supposed wrath of a Hindoo deity, the immolation of a human victim has in the present century actually taken place ; while the more rational means of explaining the communication of such pestilence, has been either overlooked or unwillingly resorted to. It is very natural that those who might be left to perish, would deny the possibility of infection by approaching the sick ; and the very idea of imparting disease, is revolting in the highest degree to our feelings. The reality of this mode of conveying the seeds of disease, seems a truth which slowly forces itself on the mind. Sydenham must have met with instances more than sufficient to prove the infectious nature of Small Pox. The power of communication by touch was long denied the Plague. Procopius doubted whether it could be conveyed by contact, and satisfied himself by some short and partial experience, that it did not spread by this means, founding his conclusion on the fact, that it had not been conveyed *στε ιατροῦ στε ιδιωτῇ*.

Cholera has, indeed, exhibited here all those singularities which have rendered its increase by contagion doubtful, and which have led those who have had ample opportunities for forming an opinion on the subject to directly opposite conclusions. I was informed by an intelligent friend, Mr. Cortazzi, who formed one of the Board of Health at Odessa, and who was free from all pre-

judice, and had no pre-conceived notions to support, (he was indeed, a merchant residing at that port) that the doctrine of non-contagion there gradually gained ground, after the disease had visited the city, and that with one single exception, all the members of the Board, all their physicians and surgeons, were finally convinced that it was not contagious.

Dr. Lefevre, whose brief sketch of the complaint is stamped by clearness of style and honesty of expression, admits, that although he saw much of the commencement of the disorder at St. Petersburg, when alone the question of contagion can be decided, further evidence was yet requisite fully to satisfy his mind.

Here, as abroad, the negative evidence was against such a means of extension, and very singular cases of exemption were noticed. More than 100 cases were admitted on board the hospital ship the "Dover," all which had occurred singly in vessels on the river, a fact, which would seem to render the possibility of communication from one person to another no longer a point of dispute, and if taken alone would appear decisive, for it is scarcely within our power to conceive a more favourable condition for the reception of a noxious exhalation than is presented by a crowded, close, ill ventilated cabin. Similar observations I may observe have been repeatedly made with respect to the Plague. Thus Russel in his history of that disease at Aleppo observes that it was rare to find more than one sick person in the same family

even after the disease had for some time prevailed. And amongst a large number of persons employed by the French to bury the dead of plague, when it raged at Rosetta, one alone took the disease.

This is doubtless extraordinary, and numerous as might be the instances adduced in support of the conclusion to which it would lead us, we must however bear in mind that one single well attested instance to the contrary would be sufficient to controvert it. But I do not intend to enter into the discussion of the contagious nature of Cholera; the acknowledgment of those who were most opposed to the doctrine of contagion, that their declarations were too general, has rendered this unnecessary—should I, however, be called upon to state any one instance which carries conviction to my mind that Cholera is contagious, I should cite the case of a merchant vessel, the “*Brutus*,” which left Liverpool, during the prevalence of Cholera, with a crew in perfect health, and who continued so, until the seventh day, when a seaman died of the Cholera; in a day or two, other cases occurred, the disease then rapidly spread amongst the crew and passengers, 113 of whom were attacked, and upwards of 80 died. This furnishes me with a conclusive argument, as no undoubted instance has ever occurred of Cholera breaking out at sea, without previous communication with an infected port, and as I shall have occasion to notice afterwards, the contagion may remain dormant longer than a week.

Unfortunately one of the powerful arguments

of the anti-contagionists “that the attendants on the sick have generally escaped,” has been disproved in our experience. Almost all those in immediate contact with the infected during life, or with their bodies after death, or employed to wash the clothes polluted by the excretions, have in a greater or less degree been affected, and but too many numbered with its victims.—Fewer medical men certainly perished in London from Cholera than from the plague in 1665, but more in proportion to the total number of deaths. I have heard of but four in the neighbourhood of London, while eight died during the plague, but the proportion at St. Petersburg was one in 5, and in some parts of this Island it was very considerable.

The mortality in cholera has in our country proved comparatively small, the number of deaths reported within the Bills of Mortality being 3,200. The total number of deaths by disease during 1832, was 28,111, a very large number, when we find that the christenings were only 26,974; and when we observe that the deaths from disease in the previous year were only 24,926, the number of christenings being then 28,263; and the number of deaths in 1832 will appear still larger if compared with 1830, when the deaths from disease amounted to 21,345, which was, however, rather below the average of the four preceding years. The perfect accuracy of the Bills of Mortality may be questioned, and it is probable that here, as under all other circumstances of pestilence, the numbers reported, fall short of the

reality, and that many cases will be returned under other heads. Thus we find that in 1832 there is a great increase under the head of inflammation of the bowels, as 604 cases are found ; while in 1831, 138 cases only were reported ; and in former years but very few. It is therefore probable that many of these were the consequences of Cholera, which I have frequently observed to terminate in this manner. Under the head of spasm too, an increase will be found of more than 100 cases above the preceding annual report. Taking, however, the returns as we find them, and calculating the numbers which perished in the population included within the Bills of Mortality, (which according to the census of 1831, is 354,253), will give us a mortality of about 1 in 110. But some of the most populous parishes of the Metropolis, (Marylebone for instance), are not included within the Bills of Mortality ; as they were but thinly peopled at the time of the establishment of those returns.

If we include the whole of the Metropolis, the population of which in 1831, amounted to 1,474,069 and calculate the total loss by Cholera at about 5000, it will give a proportion of nearly 1 death in 295. The loss of life within the jurisdiction of the city of London is but 359 in a population of 125,563, a proportion of deaths by Cholera of about 1 in 320. And it is not without its use, to take a glance for a moment at the population of the City at the time of the prevalence of the Plague, in the year 1665 ; which at that period was upwards of 300,000—more than double that of the

present day. It may also be important in considering the relative mortality of epidemic visitations to observe that the mortality which formerly was at all times 1 in 20, has from the various improvements, more complete regulations and drainage, been reduced one half. But to continue with our calculations of the mortality of this disease, the numbers reported in Great Britain has been somewhat more than 20,000, in a population of 16,260,381, so that we may consider the proportion of deaths on the whole to have been 1 in 800.

This statement would render certainly the alarms which were excited on the threatened invasion of our shores, apparently much beyond reason, but the mortality in other countries far exceeded our own, and rendered the want of proper preparation a fair matter for reproach. In India, at Penang, three-fourths of the population died, at Ponteanah one alone of the Dutch garrison escaped the contagion. At Tiflis, of the population that remained in the city, one died in three: and true as the charge may be generally of the unhealthy state of foreign towns, yet Fraser who was at Tabreez during the prevalence of this disease expresses his surprise at its extension, and says, "if we were to judge by appearances, it is difficult to conceive how an epidemic could possibly exist at this time of year, for every thing seemed to conspire to render it healthy. The country is dry and open without either wood or stagnant water to create unwholesome exhalations; the heat within doors never exceeded 84

degrees, and refreshing breezes, which prevail almost constantly from March till August, continued to blow all the time." In Russia of the population of the infected provinces one-twelfth it is calculated were attacked; in Hindostan, one-sixth, and in Persia, one-fourth; facts sufficient to render our comparative immunity a source of congratulation; and would render any neglect in precaution highly culpable, especially if we recal to our recollection the numbers who perished in former epidemics. In the last plague that visited this country, we find by Evelyn's account, that in the course of one night more than 4000 perished, and that above 60,000 certainly were destroyed. Of the Black Plague, it is reported that upwards of 23,000,000 had died in the East, not including China, where 13,000,000 are supposed to have perished, and Europe is supposed to have lost 25,000,000. Nor can the Cholera indeed be considered very inferior in extent to this plague in the countries which it has most severely visited. Before it reached China and when its ravages had been comparatively limited, in the East alone 18,000,000 are stated by Mr. Jonnes to have been thus swept away.

A subject of much interest and importance is the consideration of the probable return of this disorder. In India for more than half a century at least it has constantly prevailed, and the disease has broken out, according to above-mentioned authority, between the years 1817 and 1830, 656 times. In the province of Madras, a country in-

cluding a space of 60,000 square miles, there have been 178 irruptions. In the province of Bengal, 200. In Bombay it has prevailed 55 times. And although in Europe it has seldom returned with any severity, yet we cannot consider ourselves exempt from its recurrence when we see the intervals which have occasionally occurred abroad. Madras was free from 1818 to 1820, and again from 1824 to 1827, and the mortality has greatly varied in these different visitations. It appeared here as on the continent, that persons were seldom twice attacked, many nurses in the Cholera establishments having themselves been admitted labouring under the disease, and afterwards continued with impunity in daily and constant attendance on the sick.

Much has been said of premonitory disease ; and in India it was observed that a fever of a peculiar character preceded Cholera in various situations. The medical practitioners of Calcutta noticed the unusual prevalence of fever of a low or typhoid character ; and in various parts of Bombay, a disorder commencing with slight feverishness, but accompanied with swellings in the groin or armpits with symptoms indeed of the true plague, was very prevalent. This disease first appeared at a village in one of the provinces of Hindostan, and traversed the whole country. One village alone lost 580 persons ; and another from 16 to 1700. The disorder is stated to have been usually fatal on the fourth day ; and the proportion of *deaths* to have been greater than

in any disorder that *ever* appeared in *any country*. No such premonitory notice has preceded Cholera in other places. The fevers according to the Bills of Mortality were certainly in the year 1831 more numerous than usual, but not to any very considerable extent. The number in that year is reported to be 1,367, about 400 more than the preceding year. But in the year 1826 they were above 1000 ; so that no great increase can be said to have taken place ; and certainly no such character as that noticed in the fever in India was generally observed. A fever prevailed extensively in London in 1831, and spread through the southern counties of England, corresponding very closely with the jail or hospital fever, mentioned by Huxham, and in which glandular swellings were not unfrequently remarked ; yet this has been the case, and to a more fatal extent, without any such consequence as the late prevailing malady. And although in Liverpool a typhus fever is mentioned as having been general and destructive, yet such an event was by no means common in other situations. That form of the disease which I have mentioned as indicated by discharges of blood from the mucous membrane, seemed here to be the first in advance ; and if not entitled to be considered as the disease itself, might be called premonitory. Several cases, it may be recollected, excited some notice before the disease was proclaimed in London, and on the 16th of January a coroner's inquest sat on the body of a man at Shadwell, who died with urgent

cramps, pain in the abdomen, and vomiting of blood. This man was a seaman and had arrived from North Shields a week before. In this case the jury, guided by the opinion of several practitioners, some of whom had seen the Cholera in India, and had been appointed to the superintendence of the metropolitan districts, came to the conclusion, that the disease was not Cholera. But many cases which I saw about that time and previously, occurring in persons connected with vessels trading from Sunderland, indicated a very intimate connection with the disease in question. I may briefly mention one, which was under my own care. A man named Webster, 28 years of age, sailed from Sunderland on the 20th of January, and arrived in London about the 30th. The vessel immediately obtained pratique ; but a few days after, this man was seized with extreme pain in the epigastrium, the abdomen was forcibly drawn in, he had urgent vomiting, with coldness of the hands and paleness of the countenance : a warm bath being ordered and some castor oil given, the urgency of the symptoms was removed, and evacuations were produced free from any admixture of blood : the next morning, an inclination to empty the bowels was felt ; an enema was administered, which returned unmixed with fæcal or other matter ; but rising to go to the chair he fainted, fell back, and died. No cause could be assigned for the attack, except some slight exposure to cold incident to a seaman's life, and which the patient in this instance seemed well able to

resist, being remarkably powerful, not addicted to any excess, and living in a manner least likely to predispose to any disorder ; but he mentioned that he had had a similar attack at Sunderland six weeks before. On examination after death, 20 ounces of blood, apparently venous in character, was found in the cavity of the abdomen. The peritoneum was slightly vascular. On removing the abdominal contents, and examining the intestines externally, the stomach and the upper portion of the small intestines to the extent of five feet were natural ; when suddenly the tube was found firmly contracted, as if tied round by a thread, and from this point downwards to the iliocolic valve the intestines were of a deep purple colour. Internally, nothing at all deviating from health was remarked in the upper part of the intestines, but below the point of stricture described, the intestine was filled with dark coloured blood ; some of which was found as far as the rectum. The mucous membrane when held up to the light did not appear more vascular than usual in this part. The consistence or texture of the intestines was not altered ; although they exhaled an extremely offensive odour. This case corresponded in some degree with that already alluded to at Shadwell, but differed from it in the collection of blood in the abdomen. Another example of sudden death which occurred under my own observation in the preceding December, in a man also engaged in the Sunderland trade, who was attacked with very similar symptoms, but who

referred the pain more directly to the neighbourhood of the heart. By appropriate means he appeared to be relieved, yet on the morning following his admission to the hospital, on setting up to take his breakfast, having declared he felt himself sufficiently well to return to his duty, he suddenly sank back and immediately expired. The post mortem examination shewed the small intestines and its vessels congested; some effusion of blood having taken place in the neighbourhood of the spleen, but to no considerable extent. These cases seem fairly to be attributable to the impression of the same agency which produces Cholera, and in its most aggravated degree. Instances of death by hemorrhage I know, have taken place long before the introduction of Cholera maligna into our nosology; but I think the time when these happened and the class of persons amongst whom they occurred, justify me in considering them as other than ordinary occurrences.

Various useful district arrangements were made on the approach of this disease. But the inefficiency of the preparations for the due care of the infected was but too apparent from the first irruption of the disorder. Proper measures had been suggested by the Central Board of Health, but no hospitals for the sick had been provided: many days elapsed at Sunderland and elsewhere before the necessary arrangements were completed; and in many instances we had more reason to rejoice that the disease extended itself slowly, than

to rest satisfied with the prudence and forethought of those who, if they were not, ought to have been empowered with ample means to make all requisite provision.

These visitations at the period of their invasion while they create alarm yet suggest permanent advantages which it is to be hoped will be the result of this infliction. The great sources of the present diminution of mortality in the City of London, undoubtedly arise from the great improvements which have taken place in the widening of its streets and the comparative thinning of its inhabitants, and the probable additional increase in their necessities and comforts. In Oxford, to which city the Court removed for security during the plague in 1665, great improvements were made, in draining the city and the neighbouring ground ; by which means the city was rendered healthy : various nuisances were also removed ; for previously to that time all animals destined for the consumption of the inhabitants were slaughtered within the walls. In both these respects alterations are yet to be made in the metropolis ; it was found that in those situations where the Cholera was particularly severe, no under-ground sewers existed ; and before such an audience I need not insist on the injurious effect of the gases generated during the decomposition of animal matter, to which those are exposed who live in the neighbourhood of slaughter-houses which still are tolerated in this town. At the same time it is to be hoped that the example set

by Liverpool of removing cemeteries beyond the confines of the city, will, soon be universally adopted.

The history of Cholera clearly shews that more efficient regulations than the establishment of military cordons may be made against this malady which has defied the most rigorous enforcement of continental enactments. These have been time after time carried into effect with the utmost rigour, and have been successively abandoned, not only as inefficient to prevent the spreading of the disease, but as adding the obstruction of trade to the evils necessarily attendant on a popular calamity. When the combination of circumstances exists, in the first place, of the atmosphere to permit infection, in the second, of the person to receive it, and the actual presence in the third place of the germ of the disease itself; when all these essential circumstances are present, any infection will probably spread. The temptations afforded by commerce will always lead to the evasion of those restrictions which interfere with the interests of trade, and the intercourse of society. We cannot suppose that extensive as the fire of 1666 was known to be, that every radicle of the plague was destroyed. We cannot suppose a conflagration so mighty as to annihilate the infection of small pox, or put a stop to the re-appearance of the measles, but must look to the new order of things which resulted after the great fire, as the probable reason for the disappearance of the plague after that year. The enquiries instituted

during the prevalence of Cholera, would show that at this present moment, in the very heart of our city, a combination of circumstances might be found little differing from that which Mr. Barnes has described as existing in the native towns of Bengal. These evils which are known, and which were felt at the period of alarm, it is to be hoped will not be lost sight of now, that the alarm has passed away ; and while a thanksgiving has been offered for dangers past, we shall not be ourselves wanting in prudent anticipation of future calamities. During the period the Cholera prevailed, additional legislative enactments were made, wisely providing for the prevention of infection, as far as was practicable in the present state of society, and justifying measures necessary for public security ; the regulations which then were made should always remain in force, or if not in actual operation at all times, should at least be provided against such an emergency.

With these observations I shall conclude my Lecture of to-day, and in the next shall state the reflexions which suggested themselves to me in witnessing the various plans of treatment ; and the conclusion to which, from my own observations, I have arrived on that head.

LECTURE II.

I enter now, on the consideration of the efficiency of those means of cure which have been recommended in Cholera—From the representations of some persons, we might be induced to suppose that medicines were of no avail; on the other hand, it has been maintained, that patients were lost solely by the prejudice or obstinacy of those who were entrusted with their care, and who refused to listen to the suggestions of others, who boldly asserted that they possessed an antidote to all disorders.

While medical treatment is so eminently successful in its management of Sporadic Cholera, its inadequacy in arresting the disease in its malignant form, is but too evident; and we have yet much to discover in addition to the knowledge we already possess, of the actual nature of morbid actions; and much yet to learn, in the virtue of our remedies, before we dare assure ourselves, that we really have the power of opposing those disordered processes, which from their fatal tendency are styled malignant. The very term malignant indeed is employed to express, or at any rate to imply, an admission that they have passed

the bounds which mark the limit of our art. How shall we boast of the present state of science when half those attacked were, in general, consigned to the tomb ; and when we have to think ourselves fortunate that one in three alone fell victims in our country to this plague ?

I shall briefly enumerate some of the various remedies employed, which are so opposite as to admit of fair question with regard to the principles which regulated their administration.

To the respiratory organs different vapours were addressed, some acid, some aromatic ; factitious air was inhaled, oxygen and chlorine were breathed.

To the sanguiferous system, blood or its constituents were added ; from it blood was drawn by cupping, leeches or the lancet.

The cutaneous structure was by some wrapped in woollen ; and heat either dry or combined with moisture was applied to it by a thousand devices ; by others, on the contrary, the coldest water was declared to be the best external application.

To the nervous extremities every irritant was directed ; acupuncture, moxa, and the actual cautery.

Not less various were the remedies, or more contrary in their tendency the plans of treatment recommended for internal administration. Mild and drastic purgatives, astringents, emetics, sedatives, and stimulants were in turn employed. By some the most unrestrained indulgence in the inclination to drink was permitted, by others, it

was thought right to resist the urgent cravings of thirst.

Yet these opposite modes of treatment have met with success and hence each has found supporters, whose judgment would preclude error in the utility of the means resorted to. It becomes then a matter of no less difficulty than of importance to distinguish the circumstances when any particular plan should be recommended and to fix the period when it could be adopted, either with safety or with a prospect of success.

I do not presume to suppose that I shall be enabled entirely to classify all conflicting testimony, but should I be enabled to point out any single remedy as decidedly beneficial or to exclude any as decidedly injurious—and should I obtain the concurrence of the profession in my adoption of the one or the rejection of the other, an important practical point would be attained.

In order to simplify my arrangement I shall consider the various remedies employed, under the following heads—Bleeding,—Emetics,—Salines,—Stimulants,—Purgatives,—Sedatives.

To commence with Bleeding:—

Venesection of all the remedies which have been recommended in Cholera, is that which is most confidently spoken of and supported by the best evidence:—still in common with other remedies “Bleeding is said to have had its failure,” and well it might unless the time when it can be properly tried, and the reasons which should guide us in its employment should be more accurately specified.

“The practice of venesection, we are told, seemed to apply itself to the root of the disease by relieving the congestion of the venous system, which was invariably found loaded on examinations after death, and which congestion appeared to be the immediate cause of death. The effect of Bleeding is said to be mechanical only, and to act by removing an obstruction to the passage of blood, from the distended venous system, and if not carried far enough to remove this impediment, and allow the large vessels to empty themselves into the heart, to produce such weakness as is occasioned by the loss of blood in constitutions worn out by disease.”

Now I would ask whether in Cholera it is clearly ascertained, that the heart does stop, from pressure of a column of blood? whether a gorged venous system is the real and primary impediment to its contractions. And whether we find actually that the loss of blood does materially relieve in the manner specified?

To ascertain whether bleeding could thus relieve, I will relate a case in which it was tried with the simple purpose of disencumbering the circulation.

Edward Hide of temperate habits and having previously enjoyed good health, was admitted into St. Bartholomew's Cholera Hospital, on the 10th of September. At that time his countenance was anxious, his skin, tongue, and breath were cold; his hands felt clammy, his fingers were corrugated; he was tormented by intense thirst, urgent vomiting and severe cramps, and his

stools were frequent, copious and like rice-water. Warmth and stimuli failed to rouse him. On this a vein was opened. The blood flowed slowly, was thick and dark-coloured. No strength was acquired while the blood flowed, no rise in the pulse took place, indeed he seemed to be sinking so fast under the remedy, that I deemed it prudent to stop, when but a small quantity had flowed. His symptoms increased and he died 36 hours after admission. This case I cite as one of many in which a similar result has followed.

But venesection nevertheless has done good in Cholera. It must then operate beneficially in some way, though certainly not by relieving the circulation mechanically. Can it be by arresting the diarrhœa?

An answer to this question may perhaps be furnished by the following narration.

James Ward, æt. 56, had been for some months a patient under my care, for a cerebral affection, on board the Dreadnought, the Seaman's Hospital. On the 21st of September, he was attacked with active purging. An emetic was administered, and calomel and opium given every four hours. Neither the one nor the other checked the diarrhœa, which continued for three days, when I ordered him to lose 16 ounces of blood. The diarrhœa stopped immediately, and without further treatment the patient recovered from this attack in the bowels.

It appears then that venesection in this case arrested the diarrhœa; and it is probable that

this diarrhœa was premonitory of Cholera—as that disease was then prevailing on board the Hospital Ship and a man had died of it in an adjoining bed.

Robert Blair, æt. 32, also a patient on board the Dreadnought, had been admitted for deafness, and was attacked with violent purging on the night of the 12th of September. He applied for relief on the 13th, and then stated, that although violently seized the night before, yet, that for two days previously his bowels had been relaxed. His tongue at the time of his application was pale and cold, his eye looked dark and sunken, he complained of a sense of constriction in his chest, his pulse was labouring and irregular, he was constantly purged, and his stools were watery and copious. Sixteen ounces of blood were taken from his arm, an emetic was afterwards given, the purging which had previously occurred every half hour then ceased; no other remedy was employed, and he was discharged well of his recent attack.

In this case also it appeared that venesection arrested the diarrhœa, and that when the tongue was pale and cold, the eye dark and sunken, and the pulse feeble and irregular; here again there was great suspicion that the diarrhœa was the diarrhœa of Cholera.

Strong as the belief might be that the cases which I have cited were referable to Cholera, yet in them the characteristic discharges were absent. But I will briefly mention one of a person of the

name of Powell, 30 years of age, who consulted me on the 29th of August, about the middle of the day: he had been much exposed to the contagion, as he was assistant to the undertaker of St. Bartholomew's Hospital. He complained of vomiting, purging and cramps. For three previous days he had been troubled with diarrhœa. Five grains of calomel with a grain of opium were given immediately, and repeated in four hours. When visited in the evening the purging was incessant, the evacuations gruelly with small white flakes, but his skin was still warm, and his pulse had not yet sunk. Twenty ounces of blood were drawn from his arm, and a scruple of Hyd. \bar{c} cret. were given; the diarrhœa at once ceased, and he passed a good night. Purgative medicines were afterwards required: the case terminated favourably, and the man was discharged well on the fifth day. This case was one undoubtedly of Cholera. A large venesection was practised, and the immediate and unequivocal effect was to arrest the diarrhœa.

These cases are far from solitary, but were selected merely for the sake of illustration. The inferences which I draw from them, are:

1. That venesection will stop a diarrhœa when calomel and opium and an emetic have failed.
2. That venesection will stop the purging in Cholera before collapse has taken place.
3. That in collapse, bleeding is a remedy of doubtful benefit, if not positively hurtful.

With respect to bleeding in collapse, I have

cited but one case, as a sufficient specimen of its failure ; but I have seen others where it has been employed with the same unfortunate issue, and I have satisfied myself that we are not to look to this remedy with a view to its simple mechanical effect ; persuaded that venesection is borne worst when the obstruction to the circulation is greatest, and that it can only avail before the blood has become essentially altered, and before it stagnates and semicoagulates in the veins.

I have expressed my belief that the cases of diarrhœa which I have quoted were premonitory of Cholera ; and it may be asked, How do I know a diarrhœa to be premonitory of malignant Cholera ? It is true, that neither I, nor any one else, can determine whether that which appears to be simple diarrhœa, has any alliance with Cholera or not ; but I do know that in situations where that disorder prevails, a diarrhœa often turns out to be premonitory of Cholera, when it is least expected to be so.

An incident which I will relate afforded me a striking illustration of this assertion. A sailor applied for admission into the Dreadnought, shortly after the first appearance of the epidemic in London. He was affected with vomiting, purging and cramps. By the regulations of the ship (the case being supposed to be Cholera) was rejected, but was transferred to the floating Cholera hospital, the Dover. The disease being at that time but little known, I went on the day following to see him. He was in bed : his skin warm, his

pulse good ; Calomel and opium had been employed, and his gums were affected by it. He expressed himself as feeling much better, but still he was purged. The case was considered by the medical officer of the *Dover*, as one of simple diarrhœa, and was reported as such. A day elapsed, however, and I again visited the *Dover*. On being asked if I recollected a patient who was pointed out to me ; my reply was that I had never seen the individual before, and I was then informed that he was the person already alluded to : he had gone suddenly into the state of collapse, and was rescued from it with the greatest difficulty. In this instance, as in numberless others, it was quite impossible for any one to distinguish between the commencement of malignant Cholera and ordinary purging.

Still if we cannot arrive at a certain conclusion we may fairly presume that diarrhœa would terminate in Cholera, should it arise without obvious cause during the prevalence of malignant Cholera, should it be unusually profuse, should it be accompanied with great depression, and should it resist the means generally efficient in checking the alvine discharges. Such are the cases which I have found to be controlled by bleeding ; and my situation as Physician to the Seaman's Hospital afforded me abundant examples of them.

In support of the general use of bleeding I may again quote the observation of Mr. Cortazzi. He assured me he had witnessed its efficacy at Odessa, and therefore exempted it from the charge of

inefficiency, which he had passed on all medical treatment, but I may make an extract from his letter. He says "bleeding seldom failed when resorted to immediately after the first symptoms had appeared, and before they had fully developed themselves; half an hour or an hour after the first alarm; when resorted to at a later period, no blood would flow, or a temporary benefit afforded to the patient was succeeded by greater depression and aggravation of the symptoms. The efficacy of this treatment became shortly so evident, and such fatal consequences attended the delay which often occurred in its application, from the numerous calls of the medical men, that the Director of Police, a Colonel in the Army, took it upon himself to order all his officers and dependants to bleed, immediately on the first indication, without waiting for professional advice. This assumption of power, Mr. Cortazzi observes, was actually practised in his own house, and with such rapidity, that, although on the premises, he was not informed of the case. He expostulated warmly upon it, and moved the Board of Health to prohibit its continuance, which was done; but, he continues, I had reason afterwards to regret it, as the number of deaths immediately increased: so that upon the whole, he adds, the risk of being bled improperly gave rise to fewer accidents than the risk of not bleeding in time. The patient alluded to had all the symptoms generally considered as characteristic of the disease, and speedily recovered.

The character of the cases, the condition and habits of the people amongst whom I tried this remedy and found it beneficial, would certainly point out a certain degree of strength and power in the patient. When treating of cases of Cholera, little attempt is made to classify them ; and useful as bleeding is in general, I do not mean to assert that this treatment must not be varied according to circumstances. Venesection I have found of decided use in those cases in which the disease was incipient ; even later when other means had failed, and then advantage may be anticipated in proportion to the strength of the pulse.

Before I quit the subject of the benefit to be derived from bleeding, the chief of which I have endeavoured to shew to be in checking the diarrhœa, I will mention a case which was admitted into St. Bartholomew's Cholera Hospital, in which spasms alone were the symptom of the disease. Emma Lloyd, æt. 30, applied for relief at 10 o'clock in the morning of the 1st of September, labouring under frightful spasms which came on in paroxysms about every five minutes ; they commenced in the abdomen, drawing the muscles into hard masses ; the muscles of the chest then suffered, and the fit terminated by a forcible retraction of the head, producing sometimes complete opisthotonos. The pain occasioned was extreme. The pulse was natural ; neither vomiting nor purging was present. No obvious cause could be assigned for the attack which began on the same morning. She was not hyste-

rical, and had never had fits of any sort. She was bled to ten ounces, when the spasms immediately ceased, and did not again recur. The next object was to open the bowels, for which purpose appropriate medicines were directed. The symptoms which then presented themselves were vomiting and the most intense thirst; these were however relieved by the operation of the remedies already mentioned, but continued slightly for two days, when they left her entirely. This case I consider to be one of those mentioned by Indian practitioners as "Spasm," and of which I met with several in a milder form during the summer of 1832. My opinion was undecided when first I saw this case, but the course which it afterwards assumed, the vomiting and thirst, seemed to connect it intimately with the disorder prevailing at that time.

My belief then is, that bleeding is an important remedy in the more severe forms of the disease, either in the stage of premonitory diarrhœa, or even when it has more clearly developed itself, if resorted to before the system is exhausted by copious discharges.

Next to venesection I have ranked emetics. There are few medical men, who have watched the Cholera even in its later periods, who have not been deluded into false hopes from the obvious improvement occasioned by the full effect of vomiting. In fact, the pulse will rise, the lividity of the skin will for a while disappear, but with other unfavourable signs in a short time recur.

Numerous cases are related in which tartarized antimony given in doses sufficiently large to produce vomiting, have terminated favourably. Ipecacuanha I have seen of much use, and from first to last the use of salt emetics has been strongly recommended; many of the continental physicians, Becker of Berlin and others, thus commencing their treatment. Emetics then are of use in Cholera, but in what cases, and at what periods, it becomes matter of importance to decide. Thirty cases are related in a Letter of Sir D. Barry to have occurred in the practice of two German physicians, Drs. Ysenbeck and Brailow, (who always, however, began by bleeding) without the loss of one; and high commendation has been bestowed upon this plan by Mr. Neilson, Mr. Burton, and others in India, and Mr. Corbyn observes that the natives are in the habit of giving a strong solution of common salt in water, or the following mixture; one pice weight of onion juice, with twice its weight of arrack, both which have violent emetic effects, and it is said that they are very successful with this treatment. The benefit of an emetic was marked in the following case, which occurred under my observation.

Ann Homer, æt. 44, short in stature, plethoric, habitually short-breathed, and as I have been since informed notoriously intemperate, had been engaged to act as nurse at a Cholera Hospital, she had repeatedly been taken to her bed incapacitated for her duty by drink. Such was her condition on the evening of the 7th of September.

Next day at 5 in the morning, she was suddenly attacked with vomiting, quickly succeeded by diarrhoea. The fluid discharges from the bowels were ejected with force, and continued many minutes at a time; in the intervals between the urgent calls to evacuate the bowels, a constant draining in the bed took place; some laudanum was given by the other nurse, but which failed to check in any degree the urgency of the symptoms. I first saw her four hours and a half after the commencement of the disease. Her countenance was then shrunk, her features sharpened, her lips and hands were livid; great anxiety was expressed, and she was troubled with insatiable thirst. She was immediately bled, though not to a very large amount, as her pulse was weak and did not rise. The blood when drawn had an inky appearance. She was again visited in three hours, and the only amendment that then was perceptible, was that the draining from the bowels had ceased at once after the bleeding, and that she had had but two evacuations since. An emetic of salt and water was then exhibited, its full operation relieved the symptoms, the countenance became less livid, and perspiration took place. Some cramp, however, still remained in the extremities, purgative remedies were next employed, and all the primary symptoms ceased, a low fever resulted, and slight delirium preceded the appearance of the eruption which has been observed in some few instances in this country and occasionally abroad,

after which her return to health was slow but progressive. This person's mode of life, and her condition preceding the attack induced a decidedly unfavourable prognosis, which the character of symptoms shewed to be of the most malignant type.

Here then a malignant case was checked, and the disease was arrested by venesection, so far at least as the diarrhœa was concerned. Further measures were still requisite, and the obvious advantage of the emetic fully corroborated what had been urged in favour of this means of rousing the system, and putting a stop to the further progress of the disorder.

Those cases, I may add, in which vomiting is the chief symptom and is very urgent, may be brought successfully to a close by keeping up the action of the stomach with copious draughts of cold water, to which class of cases that simple mode of treatment seems the most applicable. I had one case under my care which terminated favourably, in which nothing else was taken in the earlier period of the disease. A young man, 18 years of age, was admitted in a state of complete collapse, having been attacked three days previously. He was in so desponding a state of mind as to lead to particular enquiry into his history. It appeared that he had been induced by his associates to join in committing a robbery which they had planned ; and having thus rendered himself an outcast from society and being tormented with the dread of immediate arrest, he

concealed himself during the day beneath the arches of the new London Bridge, leaving his hiding place at night only. His ill-gotten means of subsistence were soon exhausted ; weary of life and almost in a state of starvation, he was attacked with Cholera. The urgency of his thirst overcame all other considerations, and he crawled from his concealment to quench it at a pump in the neighbourhood ; he passed three days in this destitute situation, on the fourth his strength failing him, he fell from exhaustion, was unable to rise, and was taken by some humane persons to the hospital. I ordered him a warm salt water bath which restored the pulse. I put him on the saline plan, this, however, I was obliged soon to abandon. He passed slowly through the stage of fever, gradually recovered, and was restored not only to life, but chiefly through the active exertions of the clergyman of the parish to which he belonged, to his friends and to society.

It doubtless is extraordinary, after the lengthened discussions which have taken place on the subject of salines, that the question of their utility should yet remain undecided. Dr. Stevens's theory of malignant diseases is, that they originate in the blood ; that poisons of all sorts produce disease by mixing with that fluid, and interfering with the agency of the saline matter, which is greatly diminished, and that the appropriate treatment is to administer certain salts, which are absorbed, and thus the disease of the vital current is to be remedied. Dr. Stevens maintains that poisons

(under which head he includes specific contagions,) do not produce their effects by any influence on the brain, or by any impression made on the nervous system, but by mixing with the blood. These views whether original or not are shewn to be practically beneficial in the West Indian Fevers, in the treatment of which Dr. Stevens seems to be eminently successful, and he is led from that circumstance to infer, that the plan which he found so useful in the malignant fevers of the Western World, would prove equally efficacious in all malignant diseases; and hence that salines would be useful in Cholera. One marked difference, however, exists between the two diseases; for while an excess of acid is present in the alimentary canal, in cases of the West Indian Fever, and which Dr. Stevens considers to be the cause of the intense burning and local irritation of the bowels, in Cholera on the other hand, the dejections, as is well known, are distinctly alkaline. Dr. Stevens's theory was however simple and attractive, because it promised something of system for the treatment of a disease whose pathology was so obscure. I felt desirous of trying the method which he recommended, but I could not bring my mind to a conclusion as favourable to it as Dr. Stevens has done. I either found no benefit from it, or could not persist in its employment, the patients growing worse, or positively refusing to swallow the powders from their causing immediate and almost incessant vomiting. If the simple supply of those principles which are usually found in the

blood, and which we know to be deficient in the advanced stage of Cholera, be all that is required for its cure, we cannot have a more direct or more certain method of supplying them, than by the easy and simple process of injecting into the veins. This plan I have tried, and it was eminently successful in one of the cases in which I directed it to be practised, yet in many others it proved quite unavailing, and that too where the muscular force was still considerable, when the pulse after its employment was steady and its beat firm, and when the mind was cheerful. In cases in which those unacquainted with the disease would have felt assured that little danger was to be apprehended, I have seen the temporary respite afforded by the saline injections gradually yield to *a power*, which they had failed effectually to counteract. Unless the disease itself be arrested, or those actions which constitute it be stayed, the presence of muriate of soda and the other salts which can be artificially supplied, gives only a truce, affords a temporary suspension to a disease, which yet may prove fatal. Still it must be allowed that the use of salines is in some cases beneficial; and I have known severe cases do well under the care of others. They may be useful if given in large doses to produce their emetic effect, when the disease is advancing; they may be advantageously administered in the smaller doses, with a view of supplying the deficient salts in cases where the disease has been arrested, provided the stomach will bear them; my belief however is that in

the generality of cases other measures than the salines are more beneficial at the very outset. In the advanced periods, two essential conditions must exist to secure benefit; first, that absorption should take place in a greater ratio than the loss by dejections, and next if the saline fluid is introduced, as it may be by injection into the veins, that it should remain in the vessels; for I have seen the indisputable improvement by saline injection quickly subside from the increased discharges by the bowels, which have come on soon after the injection, and in a short time reduced the patient to his former state.

In speaking of the saline injection, I may observe that I have seen it accelerate the fatal termination of some cases, and prove the cause of death in others, by overloading the brain, and occasioning effusion into the ventricles. Still its good effects are sometimes magical, and shew that many of the symptoms in Cholera, arise from causes that can be easily remedied; but its frequent failure proves that more than the mere supply of salts is requisite to remove the disease. Dr. Stevens's plan has at least the merit of excluding others, which I believe to be highly injurious: still his remedy is by no means a specific. And what are the numbers by which Dr. Stevens claims the superiority of his plan of treatment? The deaths amongst the prisoners in the Cold Bath Fields were one in seven. And what under circumstances (as nearly similar as can be found) was the result of other modes of treatment?

Dr. Hope who resorted to instant bleeding followed by calomel and opium, lost one in 29 cases in the Mary-le-bone workhouse. Mr. James Earl did not lose a single case out of sixty, at Holywell and Earsden collieries, which he treated with calomel and opium, sinapisms and bleeding, in the incipient stage. The disease in all these cases was under medical superintendence from the beginning. It is much to be regretted that there is no diagnostic sign in Cholera, the want of some distinguishing feature having given rise to statements which mislead the unprofessional, and render it difficult to estimate the value of the various modes of treatment which are advocated by different practitioners.

The next head is "Stimulants." Now under this term very different remedies must be classed : different, not only as regards the actual nature of the substances themselves, but the manner of their action. Alcohol in all its forms and combinations has been administered. Every spirituous liquor has been recommended, and in turn used with commendation both popularly and by the profession, with manifest and real advantage. But under the head of Stimulants I shall include all those substances which are capable of exciting the minuter vessels, as well as such as produce more general effects by increasing the temperature of the body, and accelerating the impulse of the heart. Thus mercury, if given in small quantities, will rank as a stimulant, and opium in very minute doses belongs to this class. The study of

the mode of operation of the *materia medica*, shews us, that in many instances those medicines which we regard as narcotic, produce a contrary effect when given in minute portions, and some which are stimulants in small doses, become powerful sedatives if employed in larger ones. In Cholera, as in other diseases, should the result of the stimulating remedies act as a counter-irritant, advantage may be produced. Much caution is however requisite in their application, for it would perhaps be better they should be entirely prohibited than indiscriminately resorted to. That their use in some cases is paramount cannot be denied : that their employment under other circumstances is injurious seems to be no less proved ; and from my own experience I feel assured that their general recommendation has led to their too continued use, when the time for their safe employment has passed. Stimulants indeed may fail to rouse, and it may be argued that as no effect is produced, no injury can result, and certainly in the advanced periods of the disorder, the most powerful irritants do fail to produce their effect on the skin when applied to it, nitric acid is scarcely felt and barely leaves a stain, and from the want of usual effect of stimulants when taken *internally*, we may presume that a similar resistance to their impression exists in the stomach. But although we may do no harm at that moment, yet no one will approve of the employment of remedies, which are of doubtful benefit at the time, and may eventually prove in the course of the disorder

decidedly injurious. These observations will apply especially to the use of brandy, æther, and essential oils, which can only with safety be resorted to at the commencement of the disease, and then undoubtedly they have in some instances completely put a stop to all those indications, which are premonitory of Cholera.

While treating of this part of my subject, I may speak of rousing the constitution by the stimulant effect of cold suddenly applied, and this treatment I have seen usefully adopted in the later stages of the disease. Fraser, speaking of the employment of cold in cases of Cholera at Tabreez, says, "The treatment pursued by the natives is in all cases the same which we have remarked in other parts of Persia. The moment a person is attacked they seize him and drench him in cold water made colder by ice, which is abundant here, and ice is often heaped upon the stomach." Cold has been resorted to at Berlin by Dr. Caspar, in the form of effusion. The effect of this I have had repeated opportunities of observing, and from no remedies have I seen such decided and manifest amendment in the stage of collapse, except indeed from the full and powerful effect of emetics. But although the cold effusion brought on reaction, it in no way obviated the cerebral affection, one of the after-consequences of the disease, which so often resulted and proved so frequently fatal.

Should I recommend purgatives in a disorder which is characterized by numerous evacuations,

and of which copious and constant alvine dejections form a distinguishing feature, it might seem to favour the homæo-pathic doctrine, yet I am persuaded they form one of the most important remedies in this disease. But the term “purgative” must be limited in its meaning, and all cathartics are by no means to be recommended. I have seen a colocynth pill taken to remedy an obstruction in the bowels during the prevalence of Cholera, excite a diarrhœa which never ceased until life itself was exhausted. I have seen active purges themselves produce the disorder; and those which occasion copious serous evacuations are under no circumstances to be administered. Such as act simply by removing the irritating contents of the intestinal canal may be directed, and such as combine with purgative effects an astringent character, I have generally ordered; rhubarb for instance, either combined with or given shortly after a large dose of calomel. But purgative medicines, I should say, are chiefly to be resorted to early in the disease.

With respect to narcotics, the rule for their administration must be very much the same as that which I have laid down for stimulants, and although no remedies are of more consequence when properly directed, none have been more seriously abused. The abuse indeed has called into question the propriety of using them at all, and they undoubtedly have proved in *some* cases what they have been called universally, “decided poison.” Mr. Scott relates an instance, not I fear singu-

lar, in which he found a patient with a saucer containing laudanum by his bed-side ; he had been directed to take a tea-spoonful every quarter of an hour ; a direction which he obeyed until he became comatose, and shortly after expired. In another case, where 360 drops of laudanum had been taken, the same unfortunate result ensued. We are likely to be betrayed into such an error in unexpected circumstances, when those means fail which we are accustomed to employ, and when we find no other resource than by increasing the dose to endeavour to produce the desired object. If opium can (as has been asserted) in all cases restrain the discharges, I am persuaded it will often fail to do so, unless given in doses so large as to hazard the safety and life of the patient : at the same time, that if properly administered at the commencement, it is of more value than any other remedy ; but the point of time at which alone it can be properly employed must be before those actions are established, which directly tend to depress the system. It should be given in full doses, in order that the effect may be speedily produced, and if not at once beneficial, bleeding and other means should be resorted to without delay.

I have thus mentioned those remedies which I have seen in the hands of others prove beneficial, and such as in my own practice, I have found to be advantageous.

The probable reappearance of this disease will render it a matter of importance that we should have clear notions of the best and appropriate

measures to employ, when a few hours will place our patient in a situation from which no means can with certainty be advised, which are capable of affording any decided relief. Much more must be known of those processes which, taken as a whole, constitute the vital actions, and we must obtain a more thorough acquaintance with the properties of nerves, and their power of receiving and transmitting impressions, before we can be said to have a scientific knowledge of this disease ; or before we can at all consider that we are applying our resources on good and satisfactory principles. But it is my intention to consider the actual nature of Cholera in my concluding Lecture, and I shall not here anticipate what I then intend to advance, nor longer occupy your attention to-day.

LECTURE III.

IN my first Lecture, I gave a brief outline of the progress of Cholera in this country. I traced its conformity with its previous course, and stated some modifications or varieties of it which either had not occurred abroad, or had escaped observation and notice. On Wednesday I entered into the various plans of treatment recommended; and there shewed that stimulants and narcotics might be employed usefully at the commencement of the disease; and might operate beneficially, either by restraining disordered actions, or diverting them by new excitement. I then quoted instances to prove, that should these fail, we had a powerful remedy in venesection, to restrain the active determination to the bowels. I explained how salines might prove beneficial, and gave my reasons for thinking that they cannot be considered as a specific in this disease, and I expressed my conviction of the great value of some particular purgatives which I mentioned. I may take this occasion in passing, to express my belief that the constitutional action of mercury is not an antidote to the disease: for you will remember that I related one instance, in which

collapse came on, when the mouth was affected ; and in other cases which I have seen, although the simple fact of the system becoming thus acted on was in itself a good sign ; yet I could never persuade myself, that when benefit was derived, it was distinctly referable to that remedy. Nor can I subscribe to the opinion of those who rely chiefly on calomel in the almost unlimited doses in which it has occasionally been exhibited ; in some fatal cases thus treated, upon a post mortem examination, I have found the calomel unchanged in contact with the membranes, which were apparently unaffected.

I now approach that part of my subject, which has reference to the nature of the disease, the seat of a complaint which indeed seems to implicate the whole system. This research has proved a source of perplexity to men of the highest talent : and if I venture to make a suggestion explanatory of the origin of the curious phenomena witnessed in Cholera, it is with that diffidence which every one must feel, when such minds as those which have been directed to the investigation of that disease, have declared their inability to discover its immediate source.

Before I enter further into the subject, I will observe, there are many other diseases of whose precise nature, that is to say, of whose first causes, we know nothing. We know but little of the real source of that condition of the system which we call Health.—We judge from the combination of a series of complicated phenomena,

whether a person be well or ill. When all is harmonious, we are in health ; when any part is disturbed, we are unwell : and when particular organs or functions are disordered, we can give a specific name to the disease. The origin of some complaints is undoubtedly within our cognizance ; and we are by this knowledge enabled to prevent and obviate many of our afflictions. Yet the reasons of those effects which we witness are hidden from our view, both as respects the mode by which the disorders are produced, and the means which we employ to correct them. For example : we do not know any thing precisely of the nature of Malaria :—yet we consider ourselves well acquainted with Intermittent fever. We have ascertained its source ; we can predict the time of its return ; we understand its usual order ; we know its varieties ; we are aware of its consequences, and can check its progress. But can we say that we know the mode by which *any* of these results is produced ? We do not know any thing of the essential nature of Dysentery, but we are satisfied with regarding it as an atmospheric influence on a particular tissue, we study its phenomena and the best means of its cure ; and having found an efficient remedy, we consider we have attained sufficient information on the subject. We do not know why Cantharides act on the urinary and genital organs, nor why nux vomica excites the voluntary muscles, and places them beyond the controul of the will, nor why arsenic applied to any absorbent surface will produce inflammation in the

mucous membranes. Yet, although we are unable to explain why poisons exert a particular influence on individual organs, we arrive at certain conclusions from observation and experiment, and we are entitled to profess ourselves acquainted with the action of such substances. In this point of view, we may perhaps assert, that our knowledge of Malignant Cholera is not inferior to our knowledge of other disorders. Further enquiries may yet be wanting before we can satisfy ourselves of the primary changes in the disease, and the best mode of treatment is yet undetermined ; but we have certainly more accurate information of its rise, its progress, its continuance and its decline, and we have advanced further in the analysis of the fluids in this, than in any other disease. We know that Cholera is a disorder in its progress independent of seasons ; advancing in opposition to all the laws of those diseases which are engendered by simple atmospheric influence ; eluding, but not unchecked by, quarantine ; impeded but not arrested by variations in the atmosphere, a Hydra generated in the sultry swamps of Hindostan, but gifted with a fearful power of adaptation to every climate. We know that the symptoms of its presence are increased discharge from the mucous surface of the bowels, suspension of all the healthy secretions, and the cessation, or at least the depression, of the power of the heart and arteries. These are by some supposed to proceed from an alteration in the circulating fluids, and by others, from increased action, or rather

congestion in the mucous membranes. With regard to the first, the supposed primary change, that is the chemical alteration, in the circulating fluids I may observe, that it seems to me to be doubtful. The sudden manner in which those who have been exposed to the contagion will become affected, (in some cases almost instantaneously) and the rapidity with which those who have been attacked if vigorously treated, will recover; appear to afford conclusive arguments against this idea. If we are to suppose that the whole mass of the blood had become diseased; and had thus, after having undergone some essential change or vitiation, produced the symptoms I have referred to, we should surely expect more disturbance of the intellectual faculties, and more general and marked morbid alterations. Should we not expect that the blood drawn at the instant of attack would present some appearance of change in its character? But Dr. Christie, who at an early period paid attention to the analysis of the fluids, maintains, that at the commencement of Cholera, the blood suffers no change. That it afterwards becomes altered in its constituents is proved beyond all doubt by Rose of Berlin, Hermann of Moscow, Dr. Turner, and Dr. O'Shaunessey of London.

With regard to the second proposition, increased action, or congestion in the mucous membranes, being a cause of the disease, congestion may be of two kinds, it may be either inflammatory, which is an active state, or it may arise from simple dis-

tention, which is a passive state. Now, I do not find that the symptoms in cholera bear out the supposition of their being produced by inflammation. We find no local pain, we do not find that anxiety of countenance, that fiery appearance of the tongue, that excited character of the pulse, which are met with in inflammation of the mucous membrane ; the dejections do not accord with the evacuations secreted in that state, the disease does not follow the usual course of enteritis. There is no tympanitis of the abdomen, nor do the post mortem appearances correspond with the ordinary results of inflammation. In many undoubted cases of Cholera, I could perceive no morbid change in the viscera. In one case, especially, which had lasted many days, in which the symptoms were well developed, I proceeded to the examination, fully expecting to see the mucous membranes extensively diseased, and to find ample traces of disordered action. The intestines contained a dark coloured bilious fluid, which seemed to have stained the coats. They however were perfectly healthy, nor had any alteration taken place in their texture or firmness. Some red lines, indeed, were apparent on the prominent rugæ of the stomach, traces of a minute local congestion, which might have been merely accidental ; but no enlargement whatever of the mucous glands could be perceived. Dr. Christie, who considers the seat of the disease to be inflammation of the gastroenteritic mucous membranes, admits that he had sometimes perceived in these

an unnatural whiteness, instead of the redness of inflammation, and admits that he had met with cases, where no inflammation could be detected, in some, not even the slightest indication of it. The report of the Russian Board of Health at Orenburg, asserts as the result of their observation, that there is no condition of the mucous surfaces which is constant in this disease. The same doubt of an inflammatory state of the intestines in Cholera has been expressed by Annesley. Kennedy has observed that Peyer's glands and the glands of Brunner, are in many instances at least, absolutely uninjured ; and the report of the Royal Society of Medicine at Paris, admits that there is often no appreciable change in the inner coats of the intestinal canal. In general, I may say in the most rapid cases which fell under my own observation, that the mucous membranes were pulpy, white, not loaded with blood, nor were the neighbouring vessels unusually distended; the venous and arterial branches of the mesentery, on the contrary, were not unfrequently empty or nearly so.

We may then fairly doubt the real cause of this disease to be local inflammation, when neither the course of the disease, nor its progress, nor its termination, conform to the known rules of such an affection ; and when in many cases there is no trace of its effects. Is Cholera then the result of passive congestion? Are the discharges produced by an increased vitiated secretion from mere distention of vessels? Now,

congestion of vessels is a slow process, often the consequence of obstruction.—The morbid state of the vessels in Cholera is on the contrary rapidly produced.—Congestion again is a state of debility or indolence, and the secretions are consequently slowly effused, rather oozing from the vessels than freely expelled. But in Cholera we have active and violent symptoms, inordinate discharges from the intestines, accompanied with powerful and energetic action, both of the stomach and bowels. Again, the discharges in venous congestion although watery are coloured by bile, and are more or less characterized by admixture with the natural secretions, whereas these are notoriously suspended in Cholera. The rapidity then of the attack ; the urgency of the symptoms, and the muscular power which is usually exerted, appears to me an answer to this conjecture.

The seat of the disease has been referred by Caspar of Berlin to the skin, and he has much insisted upon the loss of elasticity of that tissue, as the basis of his theory. But this seems more probably a consequence than a cause, and I have myself repeatedly seen undoubted cases of Cholera in which the disease was confirmed without any affection of the skin.

Dr. Christie in his work, has referred the cause of the disease to the whole of the mucous surfaces throughout the body, and not to the mucous coat of the bowels only, and observes that the inner lining of the lungs frequently participates ; and that if it be not always affected, it is invariably

so in the severe cases. This observation does not accord with my own experience, I have frequently seen cases in which no change of structure in the mucous lining of the lungs could be perceived.

We are not assisted in our search into the cause of Cholera by reasoning from analogy with any epidemic disease which has been hitherto known; Perhaps the one which approaches nearest in its features was the *Sudor Anglicus*. In that disease there was great heat of skin, clammy and copious perspiration, great prostration of strength, irregularity of the action of the heart, with diminution of its power, intense thirst, extreme restlessness; and death resulted in three or four hours. In Cholera there is great sensation of heat in the epigastrium, and copious unnatural discharges from the mucous surface of the bowels, and in other respects corresponding symptoms. In these two diseases, although the sensation of heat was felt in different parts, and the surfaces from which the secretions were discharged were different, yet they are sympathetically allied. But in the *Sudor Anglicus* it was found that the discharges were beneficial and salutary, and the remedy chiefly consisted in their promotion. In Cholera, on the other hand, in proportion as the discharges continue unchecked the disease appears to gain strength, and the patient to sink; and there are besides in the history of the two diseases other points of difference sufficient to mark them as distinct from each other.

The appearances discovered after death in the bodies of those who have died of Cholera, are dryness of all the serous membranes, from the want of their usual lubricating secretions; absorption rapidly going on so that the features are so changed in a few hours that persons have been unable to recognize their own relations. The secretions appear to be all suspended. These consequences cannot be maintained to be the ordinary result either of hæmorrhage, inflammation or congestion. I have examined with much attention the inner linings of the arteries, and of the veins of the abdomen, and have traced their ramifications as far as instruments would serve me, and the eye could follow them, but without being able to discern any morbid change. The thoracic duct when I have examined it has been empty. The most constant appearance which I have noticed, has been a peculiar redness of the external coats of the intestines, a redness of a rosy hue, peculiar in its tint, and obviously owing to some change in the capillary arteries, differing entirely from the colour of the blood in the veins, and I have also noticed the same aspect in the transparent vessels of the peritonæum.

In the case of the man named Webster, alluded to in a former Lecture, which I consider to have been a case of Cholera, it will be remembered that 20 ounces of blood were found in the peritonæum, the transparent vessels of which must have dilated so as to permit the red globules of the blood to enter and escape. The effusion of blood into the

intestines which also took place in that case, must have been instantaneous, for his bowels had been acted upon shortly before by aperient medicines, and an enema had returned unmixed with blood immediately before the faintness came on which terminated his existence, and which marked the instant of the effusion taking place; and it will be remembered that excruciating cramps preceded the effusion, and were the chief symptom of the complaint in his case. Here then not only was there excitement of the capillary vessels, but there was beyond that dilatation to an extent sufficient to admit the passage of the red blood. In most cases I have remarked that the glands of the mesentery have been distended, and have contained a reddish fluid.

Now the impression produced upon my mind by the observations which I have been enabled to make on the causes of Cholera, has led me to the belief that the primary cause of the effects witnessed in that disease arises from an affection of the nervous system; that the immediate result of this is an irritability of the lymphatic portion of the vascular system, occasioning increased fluid discharges, which exhaust the system and prove eventually fatal. The depression of the sanguiferous system is not the first step in the disease, for although the pulse is in general depressed, yet it is by no means invariably so, for cases are occasionally met in which copious and characteristic dejections are found while the pulse continues good.

The simplest effect of the Cholera virus on the system seems to me to be illustrated by that class of cases which I have mentioned, in which spasm alone marked the commencement of the disease. This affection seems to me a strong confirmation of what I have advanced, that the nervous system is first affected. I have observed that in many instances medical men and others who approached the sick were affected with cramps in the calves of the legs and in the arms and hands, although not previously subject to any such affection. I have myself experienced this; and my colleague on board the *Dreadnought*, Mr. Lawson, afforded a striking example; and I have known many other instances. This I conceive to be attributable to the presence of the poison of Cholera in its mildest form, acting upon constitutions apparently not susceptible of further influence, and proceeding only to an extent which did not create alarm, or require treatment.

The fluids which are effused in Cholera are more than simple serum; they differ essentially from that secretion which is produced by active irritation of minute vessels, such as we see produced by the application of vesicating substances to the skin. It contains a coagulating part; fibrine is suspended in it; its appearance is milky; circumstances which distinguish it from simple serous effusion; but in every point in which it differs from serum it approaches to the usual contents of the lymphatic vessels. If the numerous discharges which are poured through the bowels were supplied

by the blood vessels alone, we might *à priori* expect to find excitement of the sanguiferous system. But that system, it is well known, is paralyzed in all its superficial branches. I have opened the temporal artery of a Cholera patient and seen no blood flow; and M. Dieffenbach of Berlin cut down upon the brachial artery and found its pulsation to have ceased; and on examination, discovered, that no circulation was carried on through it. Again, under these circumstances, viz., when the arterial system is much depressed, it is well known that hæmorrhage and profuse discharges are in general suspended; and we feel satisfied that the active processes of inflammation are put a stop to, when, having opened a vein, we feel the radial artery falter or stop. No such suspension takes place in the disease of which we now are treating. Hours after the absolute suspension of the pulse, Cholera proceeds unmitigated in its severity, and unimpeded in its course. The same causes in general which operate to depress the power of the heart and arteries increase the energy and action of the lymphatic vessels. It is not probable that the profuse discharges which are actively poured out in Cholera should be furnished from the vessels of a depressed system, to whose contents the discharges in substance bear no resemblance; but it is highly probable that these discharges should proceed from the action of the lymphatic system, which derives its energies from the depression of the sanguiferous system, more especially when the

discharges themselves are in their nature as nearly as can be judged, identically the same as the ordinary contents of the lymphatic vessels. A serious impression on that portion of the nervous system which supplies the abdominal viscera is sufficient to explain most of the phenomena which occur in Cholera. An impression upon that system is more certainly acknowledged by the heart than one made on any other. A blow on the epigastrium will instantaneously destroy life. A large dose of arsenic will produce general coldness, and destroy life by suspending the action of the heart. Another illustration of this may be adduced from the effect of nitric acid ; in a case which is related by Mons. Tartra. “ A person,” he says, “ driven by distress of mind to commit suicide, swallowed two ounces of concentrated nitric acid vomiting immediately came on ; appropriate remedies were used, and he was conveyed to the hospital. While on the road he frequently retched and stopped to drink. When visited by Mons. Tartra, the surface of his body was colder than natural, his pulse was small and frequent, and his respiration was laborious. These symptoms gradually increased ; a cold sweat bathed his brow ; his limbs became like ice ; the pulse was small and almost imperceptible ; still he could rise in bed, and make continual but useless efforts to quench his thirst and satisfy his urgent desire to make water and to go to stool. In this state he continued during the night : at last,” the account adds, “ he did pass a few drops of urine :

the shocking appearance of the body resembled that of a corpse ; but he retained his senses to the last ; and he was speaking when he expired, nineteen hours after swallowing the acid."

There is a singular coincidence in these symptoms which arose from injury inflicted on the stomach, a part supplied by the semilunar ganglia, with those which arise from Cholera. The universal coldness ; the suppression of urine ; the vomiting ; the thirst ; the clammy cold and icy sweat ; and the clearness of the intellect which remained, as in Cholera, unimpaired to the last. In relating this case, if the cause of the symptoms were omitted, I might be supposed to be describing a case of malignant Cholera. The leading features in the case quoted were strongly marked, and its duration was beyond that within which many cases of Cholera have proved fatal. Other examples might be cited of somewhat similar effects from other poisons. The suppression of urine is especially frequent. A case is mentioned in the *Journal de Medicine*, where from the effects of arsenic the urine was suppressed for several days. Sauvages, in his *Nosology*, mentions one instance where no water was passed for more than a week. The same observation has been made with regard to other poisons, such as digitalis, and nux vomica. But it seems to me that the case first related, the effect namely of the nitric acid, is best calculated to explain my idea that the effect of a powerful local action, (for the action of nitric acid is local only) may sympathetically

occasion many of the phenomena observed in Cholera. The coldness of the body may be a nervous effect, and thus also may the suppression of the secretion of urine be explained, aided no doubt by the well known fact, that that secretion is diminished whenever other discharges are profuse. The clearness moreover of the intellect is more compatible with a purely local impression than with one which, like inflammation, almost invariably involves in its later stages the impairment and disturbance of the faculties of the mind. One case struck me forcibly, and that one I shall never forget. Thomas Whitfield, a printer's pressman was admitted into the Cholera Hospital at St. Bartholomew's, being at the time of his admission on the 19th of July, in a hopeless state of collapse ; still he lived some days in a manner apparently miraculous. When all the other expedients which occurred to me had failed to restore heat and circulation, it struck me that the inhalation of vapour at a high temperature might supply warmth to the system, some fluid to the blood, and promote its healthy changes in the lungs. This was accordingly quickly put into practice. The effort to inhale the vapour, reminded him of his former habits, which in earlier life had been those of low dissipation, and he remarked that our contrivance was but a poor substitute for smoking. Here seemed a favourable moment to try the effect of tobacco smoke, a plan frequently advocated in the Cholera Gazette ; and on my patient expressing a willingness to try the experiment, the ne-

cessary arrangements were soon made ; and in doing this no time was lost, as it was obvious that he was now fast approaching the moment of his dissolution. Still he was able to make the effort, and he continued to smoke for some minutes with apparent satisfaction. There was something shocking in seeing one who appeared already dead, performing the part of a boon companion. He pledged those who stood around him ; sang part of a favourite song ; again attempted to smoke, but his lips would not obey the will, I removed the pipe from his falling hand, he sank back, and after a few convulsive expirations, ceased to exist. Who, having watched the corpse-like features and turned-back eye, has not been often surprised at the return of consciousness on excitement, and tried by vain attempts to rouse the attention, and keep the lingering spirit in its seat ? And who, that has been called upon to minister to persons thus afflicted, has not seen with a degree of awe, the sharpened features and almost rigid muscles occasionally forced into an iron smile, even in the moment of death.

The rapid and extraordinary wasting of the body in Cholera, is readily accounted for by the increased action of the absorbents, which seem especially exerted in this disease, more so than is common after the most copious effusions of blood from arteries or veins. Action of the absorbents does not take place to the same extent in cases of large and sudden hæmorrhage ; and it would require a long and wasting malady to produce

those changes which result in a few hours from Cholera. The hurried breathing may be explained as the result of simple exhaustion ; but it is more difficult to account for the want of change in the blood in its passage through the lungs. The fact is well known, that in Cholera less carbonic acid than usual is given off from the lungs, and the depression of the nervous system is cited to explain this effect. But this is a mode of accounting for a fact which a little reflection will shew to be untenable, for the nervous system, it seems, has but little share in this process, since the changes of the blood in the lungs go on in animals after they have been decapitated, and while artificial respiration is maintained. This want of alteration then in the blood appears more properly referable to its stagnation in the vessels ; or to the changes which it undergoes in Cholera, copious discharges having robbed it of some of those constituents which are essential to this process.

Although I have stated that inflammation is not the primary affection in Cholera, I must not be understood to assert that it may not occasionally be produced as the disease advances. If the medical treatment should fail to stop the symptoms in the early stages, the result of the increased action I have described, is frequently to produce inflammation of the mucous membrane. Of the symptoms, appearances and mode of treatment of this resulting inflammation I need not speak, they are well known and may be successfully combated by the usual remedies. Bleeding both

general and local will afford relief, and mercurial remedies will generally secure a favourable termination to the attack. Another termination of the disease is consecutive fever of a kind which appears new and peculiar.

The late Dr. Babington said it was to him of an anomalous character. Its aspect is typhoid; the pulse is often slow, the strength prostrated, the stomach is highly irritable; the head in different cases is variously affected, from hebetude to stupor, from slight incoherence of ideas to fierce ungovernable madness. In examining cases of this sort, I have sometimes been unable to find any change either in the structure of the brain or its membranes, or of the viscera, which could account in any way for the symptoms; and I cannot therefore consider inflammation to be the cause of this disturbance. The fact of the presence of urea in the blood in undue quantity at this stage of the disease may account for many of the symptoms. It has been noticed by others, and was very strongly marked in a case which fell under my own observation, and which as it presented a feature of great interest and novelty, viz. the presence of urea in the bile, I will relate. James Arthur, aged 52, of strong frame and temperate habits, was put under my care on the 29th of August, 1832, in a state of great exhaustion from purging with choleroïd stools, which had been passing for two days previously; he had also vomiting and cramps. I hesitated to try the saline injection, which at that time was attracting much

attention, because his pulse although weak was perceptible, and his skin still retained its warmth ; I therefore contented myself with ordering those remedies which seemed to offer the best chance of allaying the irritability of the stomach, which was his chief distress at the time. This irritability, however, was uncontrollable ; nothing remained upon the stomach : and I was obliged to leave him to his own discretion, in the selection of ingesta. Soda water alone remained upon the stomach, and on that he seemed to subsist. For a few days watery evacuations passed from the bowels, but the discharge then ceased. He remained in a composed state, was collected, nay cheerful when roused, but relapsed into a doze when not excited. For one or two nights he was slightly delirious, but he was relieved by leeches applied to his temples. Vomiting was the most unfavourable symptom present, and he constantly brought up very large quantities of bile, and bile was also discharged by stool.—No urine passed, or but a trifling quantity only during eight days, after which he gradually sank and died. When examined after death, nothing to explain the symptoms was detected. I determined therefore to preserve some of the fluids for the purpose of analysis ; and accordingly some blood was collected from the vena cava ; some urine (nearly a pint of which was found in the bladder) and some bile were also collected in clean vessels (the gall bladder containing $5\frac{1}{2}$ oz.) and forwarded to Dr. O'Shaunessy, who obligingly undertook their investigation.

The blood contained in the 1000.

Water,	702.00
Urea	3.66
Salts	6.00
Albumen	} 288.34
Fibrine	
Oily matter	
Hæmatosine	

The urine contained

Water	973.75
Urea	4.40
Albumen	19.35
Salts	2.50

And a trace of Uric acid.

Healthy blood contains at most but a trace of urea. Arthur's was loaded with a quantity which Dr. O'Shaunessy in his account says, exceeded that of any other Cholera patient he had ever examined, and this state of the blood was, in his opinion, alone sufficient to have poisoned the patient. The urine contained only four parts two-fifths of urea; a great deficiency, its healthy proportion being 30.00. The new fact however presented by this analysis was the result of the examination of the bile, which, as I have already observed, had been secreted in an extraordinary quantity during life, and had been evacuated both by vomiting and stool. This bile, although differing nothing in appearance from that in health, yet contained in 1000, 6 parts of salts, *and 3 parts of urea.*

Experiments upon animals have shewn that

after the kidneys have been removed, and the blood has consequently become loaded with urea, enormous discharges of bile take place from the intestinal canal.

The secretion and discharge of unusual quantities of bile, which was a feature in Arthur's case, is one which I have noticed frequently in the same stage of Cholera in other patients, where there was much disturbance of the functions of the brain.

Now the presence of urea in the bile may possibly serve to explain the distention of the gall bladder, which is often observed in Cholera, from irritation of the ducts, by this unnatural addition to its contents. It is interesting, moreover, as shewing the office of the liver to be one of excretion, and a means by which injurious substances in the blood may make their escape when the secretion of the kidneys is suspended, with which it may be in some measure vicarious in its office.

In those cases of Cholera in which the secretion of urine continues suspended, the fever which I have described very frequently results, and to this suppression we may ascribe the class of symptoms which this fever presents to us. Very many cases when apparently doing well, suddenly assume the low typhoid character which I have mentioned, and some will terminate fatally in a few hours, others remain in that condition for a week or more, and then with little apparent malady, or at least without any very obvious symptom of local mischief, gradually sink. In

others more excitement is found, with urgent symptoms which are referable to the state of the brain, which organ is always more or less implicated, and the form most frequently seen was that of coma. This state bears much analogy to that which is observed in *Ischuria renalis*. When the secretion of urine is suspended, coma will come on in some instances with great rapidity, at other times slowly ; but in most cases with a fatal result. The danger to be apprehended when the urine is suppressed has been admirably pointed out by Sir H. Hallford in a case cited in his orations.

That languor and nausea are symptoms which accompany diminished urinary secretion, has not escaped the notice of Heberden, who observes “ *In nonnullis qui valetudinis erant conquassatæ urina numquam reddi potuit quin statim incideret haud mediocris languor cum nausæâ.*”

In Cholera the return of the secretion of urine has justly been considered as an omen of good augury, and until that is restored we cannot entertain any well founded hope from other signs of amendment ; the case will still remain doubtful, and the practitioner who allows the friends of his patient to think him safe at this critical time will often have reason to regret his want of caution ; I have seen instances, although rarely, in which the urine was passed even at an advanced period of collapse, and cases occur where during convalescence that secretion is in some degree restored, yet without benefit. This occurrence may be possibly explained by what was noticed in the

case of the man Arthur lately alluded to—Urine it will be recollected was found in his bladder, but it was shewn to contain only four parts of urea in the thousand, when in health it contains thirty, so that it would have required an enormous quantity to have passed, to effect the necessary purification of blood, so highly overcharged with urea as the blood of this patient was shewn to have been. Thus we may understand how it might happen that water should pass yet little manifest advantage be derived.

The discharge of urine in one case under my observation appeared strictly to indicate a crisis. A man of the name of Burtenshaw was admitted into St. Bartholomew's Hospital in a state of collapse; bleeding and other means were resorted to, and he recovered from the first stage of the disease, but although his mouth was sore from mercury, he continued for some time in a state of low fever, discharges of bile like spinach, passed from his bowels, but for some days without any secretion from the kidneys. This was however at last restored, and was discharged by quarts with immediate and permanent advantage, little ailment remaining but weakness.

I have said nothing of the blueness, the most formidable feature in the disease I believe to many minds. This is often absent, and in very few instances in London have I seen it at all to be compared to cases which I witnessed down the river, where men in the prime of life, in robust health

and full vigour, were rapidly carried off, to some of which cases indeed the Russian appellation of black was strictly applicable. It is needless to say this effect is owing to the vessels being loaded with a dark coloured fluid; this condition and character of the blood may be inferred from looking at the vessels through the transparent covering of nail, where the lividness will mostly be perceived.

It only remains for me to adapt the mode of treatment which I have advocated, to the idea which I entertain of the nature of the disease. In the premonitory stage, calomel and opium; brandy and other stimulants may be exhibited, when only the nervous extremities are affected, or determination be only slightly occasioned, this may blunt the impressions, and thus render the nerves insensible to the cholera stimulus, or by general excitement to the sanguiferous system the disease may be arrested. Should however this determination continue, bleeding must be had recourse to, and this I conceive to act by emptying the vessels and allowing the increased discharge from the lymphatics to circulate through the veins, instead of being poured out through the bowels; but bleeding will by no means succeed if late resorted to, when the blood is thickened, and has lost ingredients which are essential to its circulation, and to the vital processes; at that time any diminution in its quantity gives no relief, indeed seems to hasten the fatal result. In com-

plete collapse, emetics and the cold effusion are infinitely the most powerful stimuli, and when the disease is once arrested, salines I conceive may be beneficial ; opium in very small doses may also be of use to check the irritability of the stomach ; but in the more advanced stages, when the head is bewildered, and when there is a disposition to coma, it is under no circumstances to be employed. I imagine that the saline injection does good, if the lost constituents of the blood be supplied in those cases in which exhaustion has been rapid, and the disease is disposed to cease, but it is a remedy far from innocent in its nature, and it is to be borne in mind, that it can do harm as well as good, one objection to its use is, that it terminates often in Phlebitis, but this may be obviated by gentleness in performing the operation, and by using a small flexible elastic tube.

The effect of injection is not fancifully described in saying that by its means, for a time at least the hand of death is restrained. The restoration of intellect, the return of warmth, the reanimation of the eye, and the re-appearance of life in the countenance, the departure of the lividity of the skin, and the sensation of comfort at once felt and acknowledged in almost every case, will shew that a series of important physiological effects is produced by this ready mode of administering remedies, which in some instances at least has proved a remedy of giant power, though its success in this disease has not been commensurate with the bold-

ness of the experiment. That human ingenuity will achieve much in ameliorating this new evil which has afflicted the human race, little doubt can be entertained. We have a host of important agents, whose use seems to be, as far as we can trace and apply it, to furnish us with antidotes against disease, and to assist in the great scheme of nature, by which all things with whose properties we are already acquainted, seem furnished to man for the comfort and protection of his being, and it would be impious to regard with any other feelings than those of gratitude, the many advantages we have derived from the extension of science, in opening to our view, and enabling us to employ for our benefit the treasures of the vegetable and mineral kingdoms.

I shall now, gentlemen, conclude my lectures. I have found in practice great advantages to have attended the method of treatment which I have laid down; and I must express the confidence I entertain in the power which medical treatment has in arresting this scourge, if employed within a short time from the commencement of the disease; for I have rarely found any case in which the disease was presented to me in its early stages, which did not yield successfully to medical treatment. Many subjects of importance have necessarily been omitted in these lectures, and many others have been alluded to rather than discussed: all have been treated of briefly; and for this some excuse may be found in the limited space allotted

to a lecture. It remains for me only to express my sense of gratitude for the attention with which I have been heard, and my hope that others may confirm my belief of the efficacy of the resources of our art, in the treatment of this singular and awful disease.

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